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# AN UNPUBLISHED PAPER ON LIGHT BY LORD HENRY BROUGHAM F.R.S. (1778-1868)

BY

**Paul A. TUNBRIDGE**

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As a student Henry Brougham derived as much pleasure from mathematics and physics as—in more boisterous moods—he did from twisting knockers off doors, to mention one of the “riotous sports” in which he indulged.<sup>1</sup> In his letter of 2nd June 1799 to Professor Pierre Prevost he explained that he had entered upon the study of light “as a sort of school exercise”.

Although a comparatively short one, Brougham’s scientific career was distinguished by having two papers read to the Royal Society while still in his teens. Not long after this he gave up his scientific studies to enter the legal profession where his excellent memory, self-assurance and wide culture enabled him to make rapid progress. Elected to the Fellowship of the R.S. in 1803, Brougham did not have time to pursue his earlier work on light until some fifty years later when he read a paper on this subject to the French Institute.<sup>2</sup>

As a writer Brougham’s literary productions amounted to about 130 works of which his *Lives of Men and Letters and Science in the time of George III* published in 1840 is probably the best known. As a lawyer he rapidly reached the peak of his profession. Attorney General in 1820 he was appointed Lord Chancellor on 25th November 1830 two days after his elevation to the peerage.

The first of his papers “Experiments and Observations on the Inflection, Reflection, and Colours of Light” was read to the Royal Society on 28th January 1796.<sup>3</sup> This paper was a descriptive study of a series of experiments in which flexion and reflection were discussed in a remarkably clear and well-presented exposition. This was followed soon afterwards by his second paper “Further Experiments and Observations on the Affections and Properties of Light”<sup>4</sup> which was read on 15th June 1797. In his introduction Brougham wrote:

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<sup>1</sup> Lord Brougham, *Life and Times*, i, p. 87

<sup>2</sup> He also contributed other papers, *Comptes rendus*, Nos. 30, 34, 36, 44, 46.

<sup>3</sup> *Phil. Trans.* 1796, Pt. 1, pp. 227-277.

<sup>4</sup> *Ibid.* 1797, pp. 352-385

“I am first to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds like the rest, equally with respect to refraction, reflexion, inflexion. and deflexion; thus preserving entire the same beautiful analogy in these four operations which we have hitherto remarked.”



Lord Henry Brougham F.R.S.  
(1778-1868)

These two papers prompted Pierre Prevost—who was later elected F.R.S. in 1806—to submit a paper to the Royal Society which was communicated by Sir Charles Blagden and was read on 22nd March 1798. The title of this paper was “Quelques remarques d’optique, principalement relatives à la reflexibilité des rayons de la lumière” and dealt objectively with Brougham’s submissions and in particular with his remarks on the subject of Newton’s experiments <sup>1</sup>. After defining reflexivity and discussing at some length reflection, flection, inflection, deflection and refraction his paper ended with the conclusion:

“Je finirai par observer, que l’explication que j’ai proposé, selon les principes NEWTONIENS, du phénomène, observé par M. BROUGHAM, dans la

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<sup>1</sup> Ibid, 1798, pp. 311-331

réflexion opérée par un cylindre très petit (§ 10) ne nuit pas à l'emploi que ce physicien en fait, pour expliquer les couleurs des corps naturels. Son sentiment et celui de NEWTON, à cet égard, ne sont pas en contradiction. Il n'est pas sûr que des couleurs des corps naturels ne soient produites que d'une façon; mais, qu'elle soient produites, il faut que la réflexion s'opère par chaque particule des corps, sous toute sorte d'angles. Et, je ne vois pas que M. BROUGHAM ait réussi, sous plusieurs angles variés, « à réflexion qu'il a obtenue par ses petits cylindres. Il semble qu'il ne parle, d'une manière précise, que de celle où l'angle d'incidence était d'environ 77°, et, par conséquent, fort grand ».<sup>1</sup>

In his letter to Prevost of 2nd June 1799, Brougham defended his submissions against the other's criticisms. A postscriptum informed Prevost, with supreme assurance, that he would have had his reply inserted in the *Philosophical Transactions* had he not been afraid that it was of too trifling a nature to merit the honour of a place there.

The reply of Prevost, if any, would be interesting; as Rector of the Geneva Academy and a Professor of Philosophy, he had himself published a number of papers on various scientific subjects.<sup>2</sup>

Edinburgh  
June 2 1799<sup>3</sup>

To Professor Prevost  
Geneva  
hand by Dr De la Rive<sup>4</sup>

Sir,

I beg leave to return you my thanks for the attention with which you have been pleased to honor my inquiries concerning light and colours—In some points it has been my misfortune not to meet with your assent, but it is great consolation to me that these are comparatively of trifling importance; and I trust that when you have read the few remarks with which I am about to trouble you, the grounds of our difference will be reduced to a still narrower compass.

In your tract lately published (*Phil. Trans.* 1798, Pt. 2), I find that no *real* objection whatever is urged against the two principles which I attempted by induction

<sup>1</sup> Ibid, p. 331.

<sup>2</sup> A. Cherbuliez, *La vie et les travaux de feu Pierre Prevost*, Geneva, 1839

<sup>3</sup> The letter was posted on 18.6.1799

<sup>4</sup> Gaspard de la Rive whose address in London was 20 Southampton Buildings, Holborn.

of facts to establish, viz. the different reflexivity and flexibility of light—I say no real objection: for you stated an apparent one to the doctrine of reflexivity, you have assured (and with perfect justice) that the angles of incidence (strictly so called) are always equal owing to the rays entering the sphere of reflection in different degrees, from a difference of repulsive force. Now, it is this difference of repulsive power which I call different reflexivity; and provided this be granted (as I think the facts require) I do not care what proportion is affirmed to exist between the angles—When I defined the difference of reflexibilities by the difference of these angles, I supposed the reflection to be performed, as it appears to sense, in a point; and tho' this is not mathematically just yet in all optical experiments it is supposed. Thus in considering the course of the rays thro' a refracting medium, we suppose them to move in straight lines tho' we know that they move in curves—we conceive that the divergence of the homogenous parts in a compound beam takes place at the surface; tho' we cannot doubt that the sphere of refraction extends to some distance and consequently that the different rays enter the medium at different inclinations; we measure the angle of refraction as if it were rectilinear whereas nothing can be more certain than that it is curvilinear—In like manner, when I speak of the angle of reflection being smaller in the red rays than in the violet, I suppose that the perpendicular is drawn thro' the point of incidence and that the divergence of the reflected rays from this point is compared—our difference here therefore, is merely verbal—With respect to the reflexivity of Newton, I am perfectly convinced by your statements that the objections which I (perhaps too rashly) urged against it are quite insufficient to invalidate that principle. There are, however, several objections or rather difficulties attending this subject which I should wish to have as clearly removed. Why is refraction always introduced into Sir J. Newton's experiments on this property of light? A plain metal speculum does not shew any such different reflexivity in the different rays. But this may be owing to the light having no easy passage through the first surface. Why, then, does not the anterior surface of a glass speculum or prism shew it? This case, you say, Sir I. N. has not considered. But it is perfectly evident that if we are only considering reflexion and leaving refraction wholly out of the question, the two cases, which you distinguish § 12, must, with respect to reflexivity be in fact precisely the same—and, indeed, the posterior surface of a plane glass speculum with no quicksilver will be found as unfavorable to the theory—for the reflection from it shews no such different reflexivity at any angle whatever—In short it appears that this property has some connection with the different refrangibility, since we find no trace of it where the latter does not also operate. It was the consideration of these things that first inclined me to doubt the existence of the different reflexivity and then disposed me to be satisfied with too slight proofs of the errors which appeared to have given rise to the common opinion concerning it. The few remarks on this point which I introduced into my paper were unconnected with the general theory and were, indeed, written

in haste. The two properties of reflexivity (in the two senses of the word) appear to have no connection with each other. The distinguishing characteristic of that sort which I have described, viz, it's separating the different species of light, is not to be found in the other—so that with respect to the theory which I have attempted to establish it is altogether indifferent what becomes of that kind of reflexivity described by Newton. The second part of your paper begins with the question whether the principles established for the case of reflexion explain that of flexion? You say that they explain deflexion but not inflexion; and you add that the idea of the most repulsive being the most attractive rays is at variance with the ratios of refrangibility. Yet in the next page you endeavour to accommodate the reflexivity and refrangibility, from an idea that the least attractive should be the most repulsive—Surely, this hypothetical opinion (granting it's justice in other respects, of which hereafter) is liable to the very objection that you stated immediately before—for the inflexion now becomes anomalous.

In § 17 you remark that the trajectory of the inflected ray has two points of contrary flexure—This only applies to the case of rays passing obliquely through both the spheres of flexion—(see the first figure of my first paper). When a ray is merely inflected it described a simple curve or rather a portion of a curve, whose extremities touch the lines of the ray's direction before and after flexion. The same remark applies also to deflexion.

Your observation (§ 18) that it is natural to expect a proportionality between difficulty of attraction and facility of repulsion, is surely contrary to all analogy. If as we recede from the center of action we find the attractive changed into a repulsive power, we must suppose that the body most strongly affected by the former, will continue to be most strongly affected by the latter force. Accordingly, in the cases of refrangibility and Newtonian reflexivity, this analogy holds by your own account—and in the reflexivity and flexibility which I have described, it must be allowed equally to exist, altho' indeed we cannot as yet explain the apparent anomalies between these two classes of actions. I must, however, confess, that such speculations are of little use untill the number of our facts is increased.

In § 21, you assert, that the only new analogy which I have observed, is that between the directions of the Spectra. It appears, however, that this fact is not to be considered as a single analogy for it includes, first, the observation of the different rays having different dispositions with respect to reflexion and flexion—and, secondly, the observation of the relative differences being the same in all the four operations. The first of these laws of nature might have been discovered without the other and would of itself have justly been esteemed an additional analogy.

In a second paper which I published (*Phil. Trans.* 1797—Pt 2) you will find several new analogies pointed out amongst the different operations of the force which bodies exert upon light—analogies which appear to hold still more generally than the former. The collection of such general remembrances as these seems to me



the only way in which the conjecture of Newton can be verified. His own wonderful discoveries were sufficient to give him the hint; and the farther we increase the number of our analogies, with the greater confidence may we assert the proposition—that great man would most probably not have given it in the form of a mere query, had he not also announced the idea of various flexibility in the same conjectural way. If the magnitude of his labours in optical science had not rendered him even weary of success and made him desirous of finishing his career without entering on a new field, it is certain that a very few reflections must have led him to observe the simple phaenomena which prove by a strict induction those important properties whose existence he with wonderful sagacity conjectured. He would then have found reason to state the identity of the power which acts on light not as a conjecture, but as a truth established in some degree by the only proof of which it is susceptible and liable to receive additional support from every new analogy which, in the progress of our knowledge we may discover.

To the observation in § 26, concerning the reverence with which the opinions and even the hints of such men as Newton ought always to be treated, I beg leave most heartily to subscribe. Accordingly it is only by stating facts that I would venture to express my dissent. It does not appear that the theory of it is a mere generalization of phaenomena actually observed. It is certainly an hypothesis accounting, indeed, for the appearances, but not in itself directly proved—and the circumstance which inclines me to doubt its truth is that we find the phaenomena of fringes or colour circles with dark intervals where no transmission or reflection at all takes place. For an account of the extreme similarity between the fringes by flexion and those by thin plates, in point of number, colour, disposition, proportions &c, I must refer you to my second paper. By attending to the account which I have given of the reflected spectra (in my first paper) you will perceive that I have examined them under almost every different angle, and, that, of consequence the difficulty suggested by § 27 vanishes.

I have thus, Sir, ventured to trouble you with such observations as a careful perusal of your very interesting paper suggested to me. From the explanations which I have given and the facts I have recalled to your attention, I am convinced there can no longer be any variance in our opinions upon this curious and important subject. Mathematicians and experimental philosophers have this very high pre-eminence over all other enquirers, that from the simplicity of their nomenclature and the accuracy of their definitions and first principles they are almost exempt from the evils of controversy so long as they confine themselves to their proper objects, demonstration and fact. All their disputes are easily terminated because these must have arisen either from error, speedily retracted or from misunderstanding as speedily rectified.

Allow me, Sir, again to return you my thanks for the honor of your notice. If the remarks which I have now stated are deficient in perspicuity or copiousness,

I beg you would have the goodness to ascribe their imperfection, not to inattention but to my want of familiarity with the subject from having given up, for a considerable time all such speculation. They were at first entered into rather as a sort of school exercise upon those topics which the course of my education led me to study. And less pleasing, though more necessary avocations allow me at present little prospect of ever again resuming them. I have now, indeed, more than ever, cause to look back with satisfaction upon my scientific amusements; since to them I owe the pleasure and instruction which I have received from your valuable tract; the opportunity of introducing myself to your notice—and above all, the gratification of having probably contributed to direct *your* attention and labour towards a most interesting and too much neglected department of science.

I have the honor to be

&c &c &c

Henry BROUGHAM Jun.

*P.S.* I should have inserted this letter in the *Phil. Trans.* had I not been afraid that it is too trifling to merit the honor of a place there—but by publishing it in the *Bibliothèque* of Geneva you would add another to the obligations under which your kindness has already laid me.

H. B. jun.

#### ACKNOWLEDGEMENT

I am grateful to the Director of the Public University Library Geneva for granting permission to reproduce Henry Brougham's letter.



